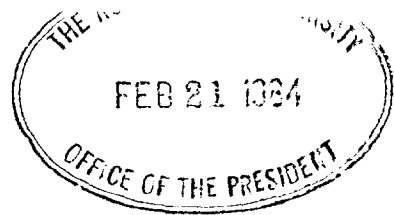


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16 February, 1984

Dr. J. Lederberg,  
The Rockefeller University,  
1230 York Avenue,  
New York, N.Y. 10021

Dear Dr. Lederberg,

I was very much interested by your letter, enclosures and enquiries of 30 December, 1984. I delayed replying, in the hope that when I had time to search in my files I might be able to reply better. This unfortunately did not happen.

Your first query is perhaps the most fascinating. My information that R.A.F. was seeking a bacteriologist to initiate genetic research at Cambridge comes from correspondence now housed with the University of Adelaide. Of course, he was a close friend of G.H. Thornton at Rothamsted and met him fairly frequently at the Royal Society, but there are also letters at this time (I believe) in which R.A.F. writes of what he has in mind, seeks advice about possible candidates and training places and mentions O. Winge in Copenhagen. Perhaps there is also a letter to Winge in the same vein, but I suspect that both Winge's name and yours occurred, in connection with training a bacteriologist in your laboratories for genetical work, in correspondence with Thornton.

Already R.A.F. must have made up his mind, and considered the possibilities of getting a qualified man, or one willing to qualify for the work. This takes us back to 1947 and to his meeting with you at Woods Hole, and your pioneering paper of 1946. One can only speculate, but it seems possible that the discussions with you might have moved his interest in recombination in bacteria to conviction. Personally, I don't think he needed to be convinced of the fact, but only that investigation of it was technically possible. Unfortunately, however, I do not think I have seen any document that can help answer your question.

Again, I have nothing to add to your observations about the use of "congression" (of genetic markers).

As for the enterprise of writing biography, I believe that anyone who has tried it would be prepared to discourse on the subject, almost indefinitely. There are, indeed many questions to be decided, not only as to the structure of the whole, or each part, but of selection, emphasis, balance. The attitude of the biographer makes an enormous difference to the overall effect; for example, I was very impressed by the way Constance

(Reid)

approached her task of writing the biography of J. Neyman, for which she had come to Berkeley and spent perhaps as much as a year with Neyman, his colleagues, and the records. Very cleverly, she gives the impression of herself as biographer (present, personally, in the biography) with Neyman close by, talking to her, smiling, with the people, the gardens, the department in the background. Partly she is reporting what she had seen and heard and read; but partly, her presence as interpreter and mediator gives ~~an~~ immediacy to her picture of Neyman, because in her writing, she seems to be responding to his charm, she seems to be convinced by his words. The subjective element in her writing is most appealing and makes Neyman most appealing. And yet, she does quote Neyman - and I noticed it in particular in reference to my father whom <sup>even</sup> she describes as "the villain of the piece" - in some of the most malicious and vile <sup>personal</sup> insinuations imaginable. Without the subjective "wash" the evidences of <sup>Neyman's</sup> self-aggrandisement by mean and vindictive behaviours (even to Egon Pearson, who had been a good friend to him) might have made the reader queasy; with it, everything is acceptable. I envy miss Reid's skill.

In contemplating a second attempt at biography, this time of W.S. Gosset, I find that very different questions arise; the portrait requires different artistic treatment. As you say the chronology offers a framework. In the case of Gosset, who was employed from his graduation until his death in one occupation (of which little is revealed by his employers), and whose 21 papers written over a 30 year period yield a very fragmentary record, not at all a developmental sequence, chronology seems of very little definitive guidance. Fortunately, there are other factors which I believe will give the biography coherence and form, individual to itself.

The subject is quite fascinating; but I am not prepared to make generalizations about it. I suppose biographers may have set down their own modus operandi. <sup>that I</sup> wonder if we biographers <sup>would</sup> come up with the same story of how we do it, or if <sup>it</sup> is very individual. It would be fun to explore further.

I must thank you very much for writing and stimulating me to think about your conundrums. I am only sorry that I have been unable to come up with any helpful information, the evidence you want; Possibly J.H. Bennet Professor of Genetics at the University of Adelaide could send you copy of the Thornton correspondence if you asked for it. Have you seen the volume of R.A.F. letters <sup>are</sup> just published, which he edited and introduced, "Natural Selection, Heredity and Eugenics, including selected correspondence of R.A. Fisher with Leonard Darwin and others," (Clarendon Press: Oxford, 1983). A very interesting collection.

With good wishes, yours sincerely,

Joan Fisher Box